PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	A Retrospective Evaluation of Healthcare Utilisation and Mortality of Two Post-Discharge Care Programmes in Singapore
AUTHORS	Ang, Ian Yi Han; Tan, Chuen Seng; Nurjono, Milawaty; Tan, Xin Quan; Koh, Choon Huat, Gerald; Vrijhoef, Hubertus; Tan, Shermin; Ng, Shu Ee; Toh, Sue-Anne Ee Shiow

VERSION 1 - REVIEW

REVIEWER	Karyn Morrissey European Centre for the Environment and Human Health, Knowledge Spa, Truro, TR1 3HD United Kingdom
REVIEW RETURNED	25-Nov-2018

GENERAL COMMENTS	Overall comment This paper presents interesting analysis of an intervention to decrease re-admissions. It found that the interventions did not have a significant impact on ADM, EM, ED, SOC & LOS may be short sighted as the analysis did not break down the results by any demographics or socio-economic factors. It may be that there are significant pre/post differences between gender, age groups or socio-economic status. Such analysis would provide important insight to the impact of interventions, given that we know older people and individuals with poor SES have higher utilization rates. It may be that the intervention did have an impact for younger or richer patients. As it stands the analysis presented is too simplistic. Whilst this is a well-structured and written paper the reliance on abbreviations distracts from the readability of the text. Please reconsider the use of abbreviations and write out the outcome variables particularly in full. Introduction In the introduction please provide an overview of the health profile and healthcare utilization rates of the Singaporean population to give context to why this study is important. Intervention Section A schematic/diagram/table of the similarities and differences between the two interventions would be very helpful. Propensity Score Matching (PSM) PSM is a well-established method to examine the outcomes of interventions. However, the section noting how the selection of multiple discharge dates as proxy POE is unclear. Please clarify.

	Should the authors not have matched patients with the same number of discharges to the control group within specific dates, as the time frames are similar thus accounting for seasonal factors? Please provide the reference for the R MatchIt PSM code in the text. Results As noted above the analysis presented is too simplistic. It found that the interventions did not have a significant impact on ADM, EM, ED, SOC & LOS may be short sighted as the analysis did not break down the results by any demographics or socio-economic factors. It may be that there are significant pre/post differences between gender, age groups or socio-economic status. Such analysis would provide important insight to the impact of interventions, given that we know older people and individuals with poor SES have higher utilization rates. It may be that the intervention did have an impact for younger or richer patients. Please provide analysis based on patient characteristics including gender, age and SES. This date is available so why was it not used? Discussion Please update discussion with the results of the new findings. Limitations Section I do not think that an RCT would have been an acceptable method to evaluation an intervention of this kind. Please provide references of papers that see matching as a positive for evaluation of interventions.

REVIEWER	Felix Holzinger Charité - Universitätsmedizin Berlin, Institute of General Practice / Institut für Allgemeinmedizin, Germany
REVIEW RETURNED	09-Jan-2019

GENERAL COMMENTS	Thank you for the opportunity to review this interesting paper. The problem of optimizing post-discharge care for patients at high risk for re-admission is largely unresolved and the current state of research could not identify a clearly effective strategy so far. General remarks Language and phrasing of the paper could benefit from some more editing, as there remain a few stylistically awkward passages and minor spelling and grammar errors. I have pointed some of these out in my remarks. The title of the paper “A retrospective quasi-experimental evaluation…” might seem somewhat misleading, as the study conducted is not really an “experiment” in my view, but a retrospective study with a secondary data control group. Abstract The term “frequent hospital admitter” for a patient who has frequent inpatient stays in a hospital seems unusual, as you normally would use the term “admitter” for the person who admits the patient (and this would be the admitting physician, not the patient). This term has been used in other publications, but all I

	could find originated from Singapore – is this term a local label, or are there other international examples? If not, you might choose another expression like “frequent inpatient hospital utilizers” for the benefit of the international reader. In the Interventions section of the Abstract, the phrasing “…manage patients’ post-discharge.” seems awkward, I would recommend to write “…manage patient’s post-discharge care.” In the conclusions section of the Abstract, you talk about “limited improvements” in healthcare utilization and costs, but as I understand the results, the evaluation showed no improvements in regard to these endpoints whatsoever, costs mostly were even higher – is there any improvement? If not, you should not use this expression. Strenghts and limitations section Third bullet point: I would recommend to writ “takes” instead of “would have taken” Firth bullet point: The study design of your retrospective study certainly is not as rigorous as an RCT, which is implicit in its design. Therefore, “may not be considered” is too weak an expression, as this is a major limitation (secondary data study) Introduction It is not clear what was changed in 2017: you describe that before 2017, NUHS RHS was one of six RHS in Singapore – but you use the past tense. What is the situation now? Was there any “restructuring” of the hospital system, as you called for such in your conclusion? The omission of the current situation is a little confusing to the reader. Methods – study design “2.3 million unique individuals” – I would delete the word “unique” here Methods – setting I would recommend the use of the term “ethnic” instead of “racial” Methods – intervention Page 8, line 8: at the end of the word “programme”, an “s” is missing Methods – matching for controls As mentioned, I would recommend the use of the term “ethnic” instead of “racial”. In my view, it is an important limitation that the matching did not take into account acute morbidity or diagnosis. Matching of patients with a large number of admissions for the NICE group is straightformward, but what about patients in NUHS-TCP who were selected for their high demand for aftercare based on their acute medical situation? Matching without consideration of diagnosis or need for care (as would not have been possible with the secondary data you utilized) at least has a major limitation here, as an important influencing factor is not considered. This should be discussed. Results - NICE IRR for EM and SOC are not significant, so I do not consider it prudent to say they “indicate higher EM and lower SOC rates”. The result is quite likely to be due to chance, or it could be due to insufficient power. This should at least be discussed.

	Discussion Page 15, line 3/5: the expression “not statistically significant different” is grammatically awkward In the same passage, you mention a study that apparently mirrors your findings on mortality, but the citation refers to a study of heart failure patients. There is a high likelihood for disease-specific effects here. In your study, we do not know the diagnoses of the population (at least in controls). Limitations The authors claim to have covered the bias from confounding factors by matching, but the effect of acute morbidity / diagnoses is not discussed. I consider this an important influencing factor that you could not control for because of the secondary nature of your data. This should be mentioned a a limitation. Implications for clinicians and policy-makers There is a call for an overhauling of the hospital system, however this is not further elaborated. In what way should it be changed? As the evidence for the effectiveness of community-centered and collaborative care post-discharge interventions is not so bad, maybe this could be a way to go? What is the role of primary care in Singapore? Is the system only hospital-based – and does this need to change?

VERSION 1 – AUTHOR RESPONSE

Reviewer(s)' Comments to Author:

Reviewer: 1

Reviewer Name: Karyn Morrissey

Institution and Country: European Centre for the Environment and Human Health, Knowledge Spa, Truro, TR1 3HD, United Kingdom

Please state any competing interests or state ‘None declared’: None declared

Please leave your comments for the authors below

Review Manuscript: bmjopen-2018-027220

Retrospective Quasi-Experimental Evaluation of Healthcare Utilisation and Costs of Two Post-Discharge Care Programmes in Singapore

Overall comment

This paper presents interesting analysis of an intervention to decrease re-admissions. It found that the interventions did not have a significant impact on ADM, EM, ED, SOC & LOS may be short sighted as the analysis did not break down the results by any demographics or socio-economic factors. It may be that there are significant pre/post differences between gender, age groups or socio-economic status. Such analysis would provide important insight to the impact of interventions, given that we know older people and individuals with poor SES have higher utilization rates. It may be that the intervention did have an impact for younger or richer patients. As it stands the analysis presented is too simplistic.

Our response: We thank the reviewer for the comprehensive review of our manuscript and for providing helpful suggestions and comments.We have incorporated the suggestions and included

additional analyses to present a more complete picture. These additions are further detailed in the revised manuscript, and summarized below:

- Additional analyses were conducted to test for the presence of interaction effects between the intervention and sociodemographic variables of gender, age group, and residential housing type.

- For all-cause inpatient admission charges, emergency admission charges, and emergency department attendance charges, the interaction term between the NICE intervention status and gender was significant.

- For SOC attendances and SOC attendance charges, the interaction term between NUHS TCP intervention status and age group was significant. For LOS, the interaction term between NUHS TCP intervention status and gender, and the interaction term between NUHS TCP intervention status and housing type were significant. For all-cause admission charges, the interaction term between NUHS TCP intervention status and housing type was significant.

- We discuss the possibility of increased case finding from increased further assessments and treatments from such intervention programmes might occur more with certain sociodemographic characteristics of the patient population.

Whilst this is a well-structured and written paper the reliance on abbreviations distracts from the readability of the text. Please reconsider the use of abbreviations and write out the outcome variables particularly in full.

Our response: We have reduced the abbreviations used in the text, particularly for the outcome variables, as well as edited the entire text to keep the use of terms consistent.

Introduction

In the introduction please provide an overview of the health profile and healthcare utilization rates of the Singaporean population to give context to why this study is important.

Our response: We have included additional information in the Introduction section that provides an overview of the health profile and healthcare utilisation of the Singaporean population, which we hope better provide the context and justification for this study.

Intervention Section

A schematic/diagram/table of the similarities and differences between the two interventions would be very helpful.

Our response: A new table (table 1) has now been added to highlight the similarities and differences between the two intervention programmes.

Propensity Score Matching (PSM)

PSM is a well-established method to examine the outcomes of interventions. However, the section noting how the selection of multiple discharge dates as proxy POE is unclear. Please clarify. Should the authors not have matched patients with the same number of discharges to the control group within specific dates, as the time frames are similar thus accounting for seasonal factors?

Our response: We have edited the paragraph to better outline the creation of multiple discharge dates for selection as controls. For all potential controls, they correspond to all discharge dates of admissions belonging to patients not in the intervention groups where the discharge dates occurred between June 2014 and December 2015. Some of these potential controls were selected as proxies for point of enrolment. A patient from the control group with multiple discharge dates would contribute

to multiple potential proxies for point of enrolment where the matching variables included, for example, the (proxy) point of enrolment and the one-year pre-enrolment healthcare utilization profile (which contained the number of discharges). As such, we have matched patients with similar number of discharges in similar time frames together.

Please provide the reference for the R MatchIt PSM code in the text.

Our response: The reference has been added.

Results

As noted above the analysis presented is too simplistic. It found that the interventions did not have a significant impact on ADM, EM, ED, SOC & LOS may be short sighted as the analysis did not break down the results by any demographics or socio-economic factors. It may be that there are significant pre/post differences between gender, age groups or socio-economic status. Such analysis would provide important insight to the impact of interventions, given that we know older people and individuals with poor SES have higher utilization rates. It may be that the intervention did have an impact for younger or richer patients. Please provide analysis based on patient characteristics including gender, age and SES. This data is available so why was it not used?

Our response: We have now included additional analyses with gender, age group, and housing type (as a proxy for SES). We found that female patients enrolled in the NICE programme had higher all-cause inpatient admission charges, emergency admission charges, and emergency department attendance charges than the female matched control patients. Patients that are ≥60 years enrolled in the NUHS TCP had lower SOC attendances while male patients enrolled in the NUHS TCP had higher LOS, compared to their respective matched controls. Patients living in Private/Other housing types enrolled in the NUHS TCP also had longer LOS and thus incurred greater all-cause admission charges than the matched control patients living in Private/Other housing types.

Discussion

Please update discussion with the results of the new findings.

Our response: We have updated the discussion section to reflect the findings from the additional analyses. We postulate that the possibility of increased case finding from increased further assessments and treatments from such intervention programmes might occur more with certain sociodemographic characteristics of the patient population, whereby the younger, female, and more well-off patients are more receptive to take follow-up action, and thus incur higher healthcare utilisation.

Limitations Section

I do not think that an RCT would have been an acceptable method to evaluation an intervention of this kind. Please provide references of papers that see matching as a positive for evaluation of interventions.

Our response: We have provided references that have used and recommended the use of such a retrospective study design with matched controls for evaluation of such interventions in the discussion.

Reviewer: 2

Reviewer Name: Felix Holzinger

Institution and Country: Charité - Universitätsmedizin Berlin, Institute of General Practice / Institut für Allgemeinmedizin, Germany

Please state any competing interests or state ‘None declared’: None declared

Please leave your comments for the authors below

Thank you for the opportunity to review this interesting paper. The problem of optimizing post-discharge care for patients at high risk for re-admission is largely unresolved and the current state of research could not identify a clearly effective strategy so far.

General remarks

Language and phrasing of the paper could benefit from some more editing, as there remain a few stylistically awkward passages and minor spelling and grammar errors. I have pointed some of these out in my remarks.

Out response: We thank the reviewer for providing great suggestions and comments.

The title of the paper “A retrospective quasi-experimental evaluation…” might seem somewhat misleading, as the study conducted is not really an “experiment” in my view, but a retrospective study with a secondary data control group.

Our response: The term quasi-experimental has been removed from the title.

Abstract

The term “frequent hospital admitter” for a patient who has frequent inpatient stays in a hospital seems unusual, as you normally would use the term “admitter” for the person who admits the patient (and this would be the admitting physician, not the patient). This term has been used in other publications, but all I could find originated from Singapore – is this term a local label, or are there other international examples? If not, you might choose another expression like “frequent inpatient hospital utilizers” for the benefit of the international reader.

Our response: We have replaced the term “frequent admitters” to “frequent hospital utilisers” in the Abstract and throughout the manuscript. We have also edited the entire text to keep the use of terms consistent within the text and with terms in the international literature.

In the Interventions section of the Abstract, the phrasing “…manage patients’ post-discharge.” seems awkward, I would recommend to write “…manage patient’s post-discharge care.”

Our response: The phrasing “…manage patients’ post-discharge” has been changed to “…manage patient’s post-discharge care” in the Abstract (and Methods section).

In the conclusions section of the Abstract, you talk about “limited improvements” in healthcare utilization and costs, but as I understand the results, the evaluation showed no improvements in regard to these endpoints whatsoever, costs mostly were even higher – is there any improvement? If not, you should not use this expression.

Our response: The conclusion of “limited improvements” in the Abstract has been changed to “no improvements”.

Strengths and limitations section

Third bullet point: I would recommend to writ “takes” instead of “would have taken”

Our response: The phrase for the third bullet point has been edited.

Firth bullet point: The study design of your retrospective study certainly is not as rigorous as an RCT, which is implicit in its design. Therefore, “may not be considered” is too weak an expression, as this is a major limitation (secondary data study)

Our response: We have edited the fifth bullet point to more strongly indicate the limitation of a retrospective study. Following the recommendation from Reviewer 1, we have also provided references that have proposed the use of retrospective study design for the evaluation of such interventions in the discussion.

Introduction

It is not clear what was changed in 2017: you describe that before 2017, NUHS RHS was one of six RHS in Singapore – but you use the past tense. What is the situation now? Was there any “restructuring” of the hospital system, as you called for such in your conclusion? The omission of the current situation is a little confusing to the reader.

Our response: Information on the merger of the NUHS RHS with the JurongHealth RHS in 2017 to form one of three RHS in Singapore has been included.

Methods – study design

“2.3 million unique individuals” – I would delete the word “unique” here

Our response: The word “unique” has been deleted.

Methods – setting

I would recommend the use of the term “ethnic” instead of “racial”

Our response: This change of term from “race”/“racial” to “ethnicity”/“ethnic” has been changed throughout the text.

Methods – intervention

Page 8, line 8: at the end of the word “programme”, an “s” is missing

Our response: This typo has been fixed.

Methods – matching for controls

As mentioned, I would recommend the use of the term “ethnic” instead of “racial”.

Our response: This change of term from “race”/“racial” to “ethnicity”/“ethnic” has been changed throughout the text.

In my view, it is an important limitation that the matching did not take into account acute morbidity or diagnosis. Matching of patients with a large number of admissions for the NICE group is straightforward, but what about patients in NUHS-TCP who were selected for their high demand for aftercare based on their acute medical situation? Matching without consideration of diagnosis or need for care (as would not have been possible with the secondary data you utilized) at least has a major limitation here, as an important influencing factor is not considered. This should be discussed.

Our response: We have further noted in the limitations section that even though patients are matched on chronic comorbidities, the matching for NUHS TCP is particularly limited without the consideration of similar diagnosis of acute medical situation and the nature of demand for care.

Results - NICE

IRR for EM and SOC are not significant, so I do not consider it prudent to say they “indicate higher EM and lower SOC rates”. The result is quite likely to be due to chance, or it could be due to insufficient power. This should at least be discussed.

Our response: This phrase has been deleted from the sentences presenting these results.

Discussion

Page 15, line 3/5: the expression “not statistically significant different” is grammatically awkward

Our response: The phrase “not statistically significant different” has been edited to “not statistically different”.

In the same passage, you mention a study that apparently mirrors your findings on mortality, but the citation refers to a study of heart failure patients. There is a high likelihood for disease-specific effects here. In your study, we do not know the diagnoses of the population (at least in controls).

Our response: We have edited the sentence to clarify that the citation refers to a study of heart failure patients, and also point out that there is a sizeable significant proportion of patients in the NICE programme with a history of heart failure. We have also included additional details in table 2 and 4 on the various comorbidities of patients enrolled in these programmes, and their matched controls.

Limitations

The authors claim to have covered the bias from confounding factors by matching, but the effect of acute morbidity / diagnoses is not discussed. I consider this an important influencing factor that you could not control for because of the secondary nature of your data. This should be mentioned a limitation.

Our response: We have further noted here in the section on limitations that even though patients are matched on chronic comorbidities, the matching for NUHS TCP is particularly limited without the consideration of similar diagnosis of the acute medical condition(s) and the nature of the demand for care.

Implications for clinicians and policy-makers

There is a call for an overhauling of the hospital system, however this is not further elaborated. In what way should it be changed? As the evidence for the effectiveness of community-centered and collaborative care post-discharge interventions is not so bad, maybe this could be a way to go? What is the role of primary care in Singapore? Is the system only hospital-based – and does this need to change?

Our response: We have further expanded that the public hospitals can foster stronger partnerships with private primary care physicians, who provide a large proportion of primary care in Singapore. The partnerships can allow for integrative co-management of patients after discharge, which would help facilitate shifting from care in hospital to care based in the community and even homes for patients.

VERSION 2 – REVIEW

REVIEWER	Karyn Morrissey University of Exeter medical school
REVIEW RETURNED	25-Mar-2019

GENERAL COMMENTS	Great work on the revised manuscript. I am now happy to accept this manuscript for publication.

REVIEWER	Felix Holzinger Charité - Universitätsmedizin Berlin, Germany
REVIEW RETURNED	25-Mar-2019

GENERAL COMMENTS	Thank you for your detailed and through response to my earlier review comments. All my points and suggestions were covered comprehensively and clearly in the revision and rebuttal letter.